



EGUsphere, author comment AC2  
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## Final response

Aubin Thibault de Chanvalon et al.

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Author comment on "Influences of manganese cycling on alkalinity in the redox stratified water column of Chesapeake Bay" by Aubin Thibault de Chanvalon et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-428-AC2>, 2022

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Dear Editor Middelburg,

Both reviewers did a detailed reading of the manuscript providing important points to improve it. Both convince us about the necessity to improve the form, including 1) to split results from discussion, 2) to improve the model description, 3) to include comparison with other studies in the discussion. This will clarify which interpretation depends on the model's hypothesis. It will also result in a more detailed section 2.3.3 underlining that the model describes the net result of sequences of reactions that occur in a water mass from its equilibrium with the atmosphere to the sampling point in order to identify *the more likely preponderant* reactions. It justifies the term *net reaction*, that the model takes into account only the reagents with substantial amounts, that it does not require a detail investigation of ongoing kinetics and that it only identifies preponderant reactions, despite minor one, such as nitrification can occur.

On the substance, we do not consider that the criticisms from reviewers #1 and #2 break down our demonstration that only a reaction producing metal carbonate is able to generate a TA/DIC ratio of 2.4. We already answered to general comments from reviewers #1. For its part, reviewer's #2 comments focus first on the kinetics. As noted previously, the model describes the net reactions having already modified the chemistry of the water mass since its last equilibrium with the atmosphere, independent to the rate at which these reactions have occurred. Reviewer #2 considers that the terminology proposed by Soetaert et al (2007) is as generic as the equation (4). The Soetaert et al (2007) publication was a fantastic source of inspiration, nevertheless their definition of alkalinity is based on the excess of negative charge plus/minus six specific chemical species which do not allow the reader to easily understand how new species (such as reduced FeS clusters or DOM) will change the alkalinity, which is what our equation (4) does. Finally, the main argument of reviewer #2 is that "ratios of TA/DIC exceeding 2 have been discussed in earlier works". Unfortunately she/he does not give any references and despite additional literature survey (including Lukawska-Matuszewska, 2016; Hu et al., 2010; Drupp et al., 2016; Kuliński et al., 2014; Abril et al., 2003; Berner et al., 1970; Hiscock and Millero, 2006; Goyet et al., 1991; Rassmann et al., 2020), we were not able to find publications reporting  $\Delta\text{TA}/\Delta\text{DIC}$  ratios above 2.

Whatever will be your final decision, we wanted to thank both reviewers for the improvements they proposed and the scientific stimulation it produced.

Sincerely,

Aubin Thibault de Chanvalon